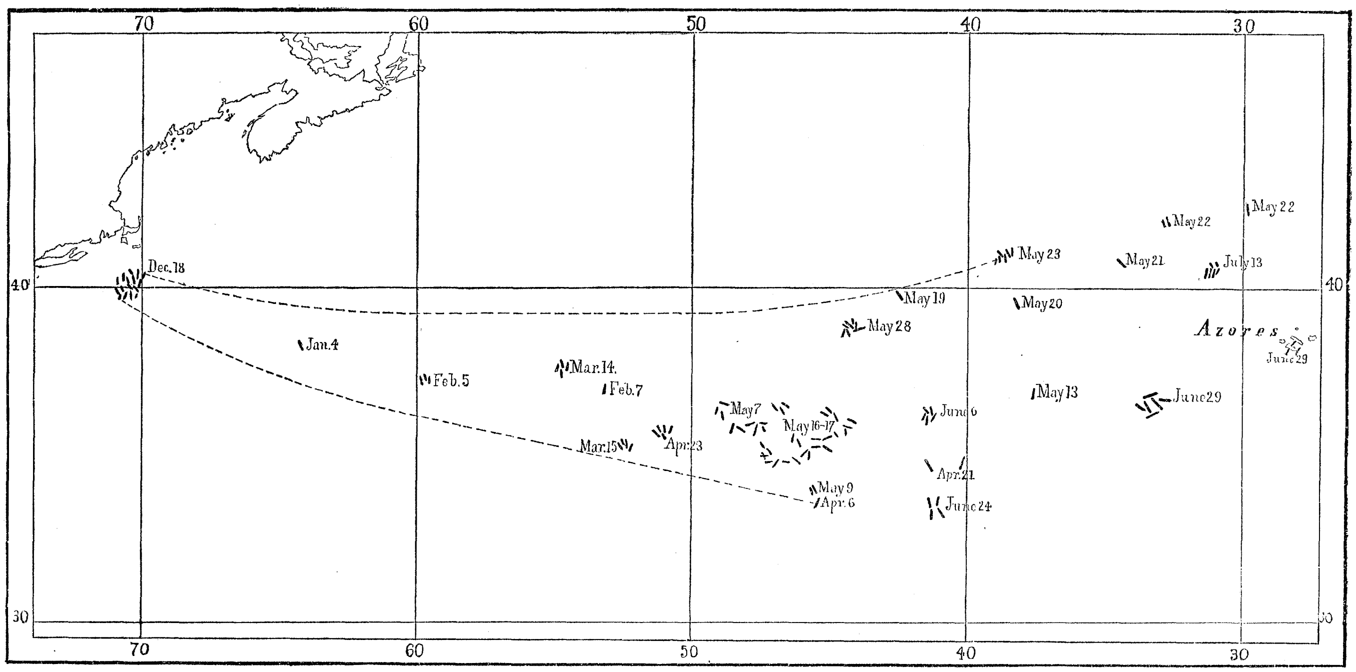
SCIENCE

FRIDAY, AUGUST 17, 1888.

THE PILOT CHART of the North Atlantic Ocean for August, issued by the Hydrographic Office under direction of Commodore John G. Walker, chief of Bureau of Navigation, is accompanied by a supplement containing a large amount of useful and interesting information concerning derelicts and wreckage on the high seas, with a graphic and complete record of the tracks followed by some of the most notable derelicts reported on back numbers of the chart. Most noticeable of all, and of especial interest at the present time, is the complete history, up to date, of the great log raft abandoned off Nantucket last December, with a tabular statement of every report received from masters of vessels since that time, of logs from the great raft. This table contains 134 reports, and although a few of them relate to timber from vessels' deck loads, yet the great majority are undoubtedly reports of fragments of the log raft. The graphic representation of the manner in which these obstructions to navigation have spread over the Atlantic is very impressive: their general drift was at first about south-east, under the influence of the prevailing north-westerly winds, and then almost due east in the Gulf Stream, the plotted tracks reaching well over to the Azores, where, indeed, one of the logs was towed into port on June 14, according to the United States consul at Fayal. Some of them are now to the northward of the Azores, drifting north-easterly, and others to the southward, drifting south-easterly, and of the former some may yet reach the shores of Europe. A considerable amount of driftwood was observed farther north, but, from the descriptions, it appears that it did not belong to the raft. That portion of the map referring to the gradual dispersion of this mass of timber has been reproduced above.



The almost world-wide notoriety achieved by this great log raft lends emphasis to the following remarks, quoted from the chart itself: "To the student of ocean currents, it is interesting to watch the tracks which the scattered logs from this great raft have followed, drifting, as they do, under the combined and varying influence of wind, tide, and current, and every log offering some slight difference of resistance to each, according to its size, weight, and depth of flotation. To the practical navigator, however, it will be of still greater interest to have logs shipped in the usual way, or at least more securely than was done in this case, in order that dangerous obstructions may not be added in this wholesale manner to those which, in the ordinary course of things, he has to guard against." The tracks of derelict vessels are also of great interest, and clearly illustrate how long these dangerous obstructions often remain afloat. For instance, one of them drifted 2,800 miles, and another the enormous distance of 3,500 miles (from off the capes of Chesapeake Bay to the Bay of Biscay, by a circuitous route).

The Hydrographic Office, of which Lieut. G. L. Dyer, U.S.N., is now in charge, is enabled to collect and publish data of this kind in complete and reliable form by means of the facilities of the branch hydrographic offices established in our principal seaports, the establishment of which has been of the greatest assistance to our mercantile marine, and has greatly strengthened the navy in their estimation. The Pilot Chart itself for August shows a new feature, which will make it of still greater value. It contains the tracks of all the notable August hurricanes on record, thus indicating at a glance both the regions where they are liable to be encountered and the general direction of the paths which they follow. A descriptive article on the chart calls attention to the fact that August is the great hurricane month, and describes the tracks followed by the two memorable hurricanes of August of last year, one of which is perhaps the most notable on record, beginning near the Cape Verde Islands, off the coast of Africa, and thence traversing the entire ocean to the westward, curving to the northward past Cape Hatteras, and thence about east-north-east across the Grand Banks, and re-crossing the Atlantic towards the British Isles and northern Norway. In this way the good work of the office is kept up.

THE INTERNATIONAL CONGRESS OF AMERICANISTS.

IT is now two years since the sixth meeting of the International Congress of Americanists was held at Turin. The next meeting is going to be held at Berlin from the 2d to the 5th of October. Before the adjournment of the Turin meeting an organizing committee was appointed, which, in agreement with the bureau of the Turin session, proposes the following subjects for the discussion of the congress. The first day of the meeting will be devoted to the history of the discovery of America, to the pre-Columbian history of the continent, and to American geology. Among the important subjects proposed for this day is a discussion of the early history of Central America, more particularly of the nationalities living there before the invasion of the Aztecs and other northern tribes, and of the chronology of the invasions of uncivilized tribes into Mexico. Professor Guido Cora of Turin will report on the publication of documents referring to Columbus, incident to the celebration of the fourth centenary of the discovery of America, and on the origin of the name of America. Mr. Gelcich, who recently published in the Journal of the Berlin Geographical Society an elaborate study of the life of Columbus, will report on recent researches in this field.

The second day will be devoted to the discussion of archæological questions. Of course, the most prominent of these is the comparison of American and Asiatic relics; and the similarity and dissimilarity of American and Asiatic jade implements and pottery will be discussed.

On the third day the anthropology and ethnology of America will be treated. Prof. R. Virchow will report on the anthropologic classification of the ancient and modern inhabitants of America and on a craniological atlas. It is to be hoped that this important work will be materially furthered by the researches of the congress. Another problem not inferior in importance to the former is that of the ethnological atlas of America, to which the Bureau of Ethnology of Washington has made a contribution of the greatest value. While the discussion of the congress will hardly add any thing to the facts referring to North America collected by the scientists at Washington, our knowledge of the distribution of tribes of South America will undoubtedly be materially increased. While these two questions refer to material to be collected, a number of others will treat the ethnological problems of our continent. Prof. A. Bastian will illustrate the theory of geographical provinces by the ethnology of America. Profs. C. Fritsch and Guido Cora will discuss the unity of the American aborigines by studying their anthropological features, and the latter will compare the diluvial human remains with those of the Indians. Professor Virchow will compare the artificial deformations of skulls practised in America with those found in Asia, Europe, and on the islands of the Pacific Ocean. Another problem of general interest will be treated by A. Krause, — the question of a connection between Asiatic races and the natives of the north-west coast of America.

The last day of the session will be devoted to linguistics and paleography. The question will be discussed whether there exists any characteristic feature common to all American languages. Another subject of general interest, upon which Prof. L. Steinthal will make a report, is the question if any similarity exists between Polynesian and north-west American languages.

A detailed programme will be published about the middle of September, and members are requested to send their manuscripts, or the titles of their communications, to the bureau of the congress before Sept. 15. The bureau is in the Royal Ethnological Museum of Berlin, which will also form one of the principal attractions of the coming congress. There are few collections in Europe which represent the ethnology of America so well as that of Berlin, and none has collections of equal value from the civilized races of ancient America. Fortunately the collections have been recently transferred to a new and magnificent building, where they will be accessible to the visitors of the congress. There are a number of old collections from the central part of South America showing the beautiful feather-work of the Indians of those regions, but the student will principally be interested in Von den Steinen's collections from the Xingu River. This distinguished explorer will report to the congress on his recent expedition, from which he has just returned. The ancient civilization of Peru, which forms one of the objects of discussion, is represented by valuable collections in the museum, particularly the great collection of pottery and gold ornaments of Macedo and that of Reiss and Stübel, which contains, besides specimens of pottery, numerous mummies, beautiful samples of woven clothing, etc. The collections from Central America date back to the travels of Alexander von Humboldt; but since that time numerous new collections have been added, principally those of Bastian and of Strebel. Last, we have to mention the extensive collections from British Columbia and Alaska.

It is to be expected that the approaching congress will materially further the study of American archæology and ethnology.

THE HISTORY OF A DOCTRINE.[1]

"MAN, being the servant and interpreter of nature, can do and understand so much, and so much only, as he has observed, in fact or in thought, of the course of nature. Beyond this he neither knows any thing nor can do any thing." — BACON'S *Novum Organum*, aphorism I.

IN these days, when a man can take but a very little portion of knowledge to be his province, it has become customary that your president's address shall deal with some limited topic, with which his own labors have made him familiar; and accordingly I have selected as my theme the history of our present views about radiant energy, not only because of the intrinsic importance of the subject, but because the study of this energy in the form of radiant heat is one to which I have given special attention.

Just as the observing youth, who leaves his own household to look abroad for himself, comes back with the report that the world, after all, is very like his own family, so may the specialist, when he looks out from his own department, be surprised to find that, after all, the history of the narrowest specialty is amazingly like that of scientific doctrine in general, and contains the same lessons for us. To find some of the most useful ones, it is important, however, to look with our own eyes at the very words of the masters themselves, and to take down the dusty copy of Newton, or Boyle, or Leslie, instead of a modern abstract; for, strange as it may seem, there is something of great moment in the original that has never yet been incorporated into any encyclopædia, something really essential in the words of the man himself which has not been indexed in any text-book, and never will be.

It is not for us, then, here to-day, to try

> "How index-learning turns no student pale,
> Yet holds the eel of science by the tail;"

but, on the contrary, to remark that from this index-learning, from these histories of science and summaries of its progress, we are apt to get wrong ideas of the very conditions on which this progress depends. We often hear it, for instance, likened to the march of an army toward some definite end; but this, it has seemed to me, is not the way science usually does move, but only the way it seems to move in the retrospective view of the compiler, who probably knows almost nothing of the real confusion, diversity, and retrograde motion of the individuals comprising the body, and only shows us such parts of it as he, looking backward from his present standpoint, now sees to have been in the right direction.

I believe this comparison of the progress of science to that of the army which obeys an impulse from one head has more error than truth in it; and, though all similes are more or less misleading, I would almost prefer to ask you to think rather of a moving crowd, where the direction of the whole comes somehow from the independent impulses of its individual members, not wholly unlike a pack of hounds, which, in the long-run, perhaps catches its game, but where, nevertheless, when at fault, each individual goes his own way by scent, not by sight, some running back and some forward; where the louder-voiced bring many to follow them, nearly as often in a wrong path as in a right one; where the entire pack even has been known to move off bodily on a false scent; for this, if a less dignified illustration, would be one which had the merit of hav-

[1] Address before the American Association for the Advancement of Science, at Cleveland, O., Aug. 15, 1888, by Prof. S. P. Langley, the retiring president of the association.

ing a considerable truth in it, but one left out of sight by the writers of books.

At any rate, the actual movement has been tortuous, or often even retrograde, to a degree of which you will get no idea from the account in the text-book or encyclopædia, where, in the main, only the resultant of all these vacillating motions is given. With rare exceptions, the backward steps — that is, the errors and mistakes, which count in reality for nearly half, and sometimes for more than half, the whole — are left out of scientific history; and the reader, while he knows that mistakes have been made, has no just idea how intimately error and truth are mingled in a sort of chemical union, even in the work of the great discoverers, and how it is the test of time chiefly which enables us to say which is progress when the man himself could not. If this be a truism, it is one which is often forgotten, and which we shall do well to here keep before us.

This is not the occasion to review the vague speculations of the ancient natural philosophers from Aristotle to Zeno, or to give the opinion of the schoolmen on our subject. We take it up with the immediate predecessors of Newton, among whom we may have been prepared to expect some obscure recognition of heat as a mode of motion, but where it has been, to me at least, surprising, on consulting their original works, to find how general and how clear an anticipation of our modern doctrine may be fairly said to exist. Whether this early recognition of the atomic and vibratory theories be a legacy from the Lucretian philosophy, it is not necessary to here consider. The interesting fact, however it came about, is the extent to which seventeenth-century thought is found to be occupied with views which we are apt to think very recent.

Descartes, in 1664, commences his 'Le Monde' by a treatise on the propagation of light, and what we should now call radiant heat, by vibrations, and further associates this view of heat as motion with the distinct additional conception that in the cause of light and radiant heat we may expect to find something quite different from the sense of vision or of warmth; and he expresses himself with the aid of the same simile of sound employed by Draper over two hundred years later. The writings of Boyle on the mechanical production of heat contain illustrations (like that of the hammer driving the nail, which grows hot in proportion as its bodily motion is arrested) which show a singularly complete apprehension of views we are apt to think we have made our own; and it seems to me that any one who consults the originals will admit, that, though its full consequences have not been wrought out till our own time, yet the fundamental idea of heat as a mode of motion is so far from being a modern one, that it was announced in varying forms by Newton's immediate predecessors, by Descartes, by Bacon, by Hobbes, and in particular by Boyle, while Hooke and Huyghens merely continue their work, as at first does Newton himself.

If, however, Newton found the doctrine of vibrations already, so to speak, "in the air," we must, while recognizing that in the history of thought the new always has its root in the old, and that it is not given even to a Newton to create an absolutely new light, still admit that the full dawn of our subject properly begins with him, and admit, too, that it is a bright one, when we read in the 'Optics' such passages as these: —

"Do not all fixed bodies, when heated beyond a certain degree, emit light and shine, and is not this emission performed by the vibrating motions of their parts?" And again: "Do not several sorts of rays make vibrations of several bignesses?" And still again: "Is not the heat conveyed by the vibrations of a much subtler medium than air?"

Here is the undulatory theory; here is the connection of the ethereal vibrations with those of the material solid; here is "heat as a mode of motion;" here is the identity of radiant heat and light; here is the idea of wave-lengths. What a step forward this first one is! And the second?

The second is, as we now know, backward. The second is the rejection of this, and the adoption of the corpuscular hypothesis, with which alone the name of Newton (a father of the undulatory theory) is, in the minds of most, associated to-day.

Do not let us forget, however, that it was on the balancing of arguments from the facts then known that he decided, and that perhaps it was rather an evidence of his superiority to Huyghens, that apprehending before the latter, and equally clearly, the undulatory theory, he recognized also more clearly that this theory as then understood failed utterly to account for several of the most important phenomena.

With an equally judicial mind, Huyghens would perhaps have decided so too, in the face of difficulties, all of which have not been cleared up even to-day.

These two great men, then, each looked around in the then darkness as far as his light carried him. All beyond that was chance to each; and fate willed that Newton, whose light shone farther than his rival's, found it extend just far enough to show the entrance to the wrong way. He reaches the conclusion that we all know; and with the result on other men's thought, that, light being conceded to be material, heat, if affiliated to light, must be regarded as material too, for we may see this strange conclusion drawn from experiments of Herschel a century later.

It would seem that the result of this unhappy corpuscular theory was more far-reaching than we commonly suppose, and that it is hardly too much to say that the whole promising movement of that age toward the true doctrine of radiant energy is not only arrested by it, but turned the other way; so that in this respect the philosophy of fifty years later is actually farther from the truth than that of Newton's predecessors.

The immense repute of Newton as a leader, on the whole so rightly earned, here leads astray others than his conscious disciples, and, it seems to me, affects men's opinions on topics which appear at first far removed from those he discussed. The adoption of phlogiston was, as we may reasonably infer, facilitated by it, and remotely Newton is perhaps also responsible in part for the doctrine of caloric a hundred years later. After him, at any rate, there is a great backward movement. We have a distinct retrogression from the ideas of Bacon and Hobbes and Boyle. Night settles in again on our subject almost as thick as in the days of the schoolmen, and there seems to be hardly an important contribution to our knowledge, in the first part of the eighteenth century, due to a physicist.

"Physics, beware of metaphysics," said Newton, — words which physicists are apt so exclusively to quote, that it seems only due to candor to observe that the most important step, perhaps, in the fifty years which followed the 'Optics,' came from Berkeley, who, reasoning as a metaphysician, gave us during Newton's lifetime a conception wonderfully in advance of his age. Yet the 'New Theory of Vision' was generally viewed by contemporary philosophers as only an amusing paradox, while "coxcombs vanquish[ed] Berkeley with a grin;" and this contribution to science, — an exceptional if not a unique instance of a great physical generalization reached by *a priori* reasoning, — though published in 1709, remains in advance of the popular knowledge even in these closing years of the nineteenth century.

In the mean time a new error had risen among men, — a new truth, as it seemed to them, and a thing destined to have a strong reflex action on the doctrine of radiant energy. It began with the generalization of a large class of phenomena (which we now associate with the action of oxygen, then of course unknown), — a generalization useful in itself, and accompanied by an explanation which was not in its origin objectionable. Let us consider, in illustration, any familiar instance of oxidation, and try to look first for what was reasonable in the eighteenth-century views of the cause of such phenomena.

A piece of dry wood has in it the power of giving out heat and light when set on fire; but after it is consumed there is left of it only inert ashes, which can give neither. Something, then, has left the wood in the process of becoming ashes; virtue has gone out of it, or, as we should say, its potential energy has gone.

This is, so far, an important observation, extending over a wide range of phenomena, and, if it had presented itself to the predecessors of Newton, it would probably have been allied to the vibratory theories, and become proportionately fruitful. But to his disciples, and to chemists and others, who, without being perhaps disciples, were like all then, more or less consciously influenced by the materiality of the corpuscular theory, it appeared that this also was a material emanation, that this energy was an actual ingredient of the wood, — a crudeness of conception which seems most strange to

us, but is not perhaps unaccountable in view of the then current thought.

I have said that the progress of science is not so much that of an army as of a crowd of searchers, and that a call in a false direction may be responded to, not by one only, but by the whole body. In illustration, observe that during the greater part of the entire eighteenth century this doctrine was adopted by almost every chemist and by most physicists. It had quite as general an acceptance among scientific men then as the kinetic theory of gases, for instance, has now, and, so far as time is any test of truth, it was tested more severely than the kinetic theory has yet been; for it was not only the lamp and guide of chemists, and to a great extent of physicists also, but it remained the time-honored and highest generalization of chemico-physical science for over half a century, and it was accepted not so much as a conditional hypothesis as a final guide and a conquest for truth which should endure always. And now where is it? Dissipated so utterly from men's minds, that, to the unprofessional part of even an educated audience like this, 'phlogiston,' once a name to conjure with, has become an unmeaning sound.

There is no need to insist on the application of the obvious moral to hypotheses of our own day. I have tried to recall for a moment all that 'phlogiston' meant a little more than a hundred years ago, partly because it seems to me, that, though a chemical conception, physics is not wholly blameless for it, but chiefly because before it quitted the world it appears to have returned to physics the wrong in a multiplied form by generating an offspring specially inimical to true ideas about radiant heat, and which is represented by a yet familiar term. I mean 'caloric.'

This word is still used loosely as a synonyme for heat, but has quite ceased to be the very definite and technical term it once was. To me it has been new to find that this so familiar word 'caloric,' so far as my limited search has gone, was apparently coined only toward the last quarter of the last century. It is not to be found in the earliest edition of Johnston's Dictionary, and, as far as I can learn, appears first in the corresponding French form in the works of Fourcroy. It expressed an idea which was the natural sequence of the phlogiston theory, and which is another illustration that the evil which such theories do lives after them.

'Caloric' first seemingly appears, then, as a new word coined by the French chemists, and meant originally to signify the unknown cause of the sensation heat, without any implication as to its nature. But words, we know, though but wise men's counters, are the money of fools; and this one very soon came to commit its users to an idea which was more likely to have had its origin in the mind of a chemist at that time than of any other, — the idea of the cause of heat as a material ingredient of the hot body; something not, it is true, having weight, but which it would have been only a slight extension of the conception to think might one day be isolated by a higher chemical art, and exhibited in a tangible form.

We may desire to recognize the perverted truth which usually underlies error, and gives it currency, and be willing to believe that even 'caloric' may have had some justification for its existence; but this error certainly seems to have been almost altogether pernicious for nearly the next eighty years, and down even to our own time. With this conception as a guide to the philosophers of the last years of the eighteenth century, it is not, at any rate, surprising if we find that at the end of a hundred years from Newton the crowd seems to be still going constantly farther and farther away from its true goal.

Although Provost gave us his most material contribution about 1790, we have, it seems to me, on the whole, little to interest us during that barren time in the history of radiant energy called the eighteenth century, — a century whose latter years are given up, till near its very close, to bad *a priori* theories in our subject, except in the work of two Americans; for in the general dearth at this time, of experiments in radiant heat, it is a pleasure to fancy Benjamin Franklin sitting down before the fire, with a white stocking on one leg and a black one on the other, to see which leg would burn first, and to recall again how Benjamin Thompson (Count Rumford) not only weighed 'caloric' literally in the balance and found it wanting, but made that memorable experiment in the Munich founderies which showed that heat was perpetually and without limit created from motion.

It was in the last years of the century, too, that he provided for the medal called by his name, and which, though to be given for researches in heat and light, has, I believe, been allotted in nearly every instance to men, who, like Leslie, Malus, Davy, Brewster, Fresnel, Melloni, Faraday, Arago, Stokes, Maxwell, and Tyndall, have contributed toward the subject of radiant energy in particular.

We observe that till Rumford's time the scientific literature of the century scarcely considers the idea even of radiant heat, still less of radiant energy; so that we have been obliged here to discuss the views of its physicists about heat in general, heat and light in most eighteenth-century minds being distinct entities. We must remember, then, to his greater honor, that the idea of radiant heat as a separate study has before Rumford scarcely an existence; all the ways for pilgrims to this special shrine of truth being barred, like those in Bunyan's allegory, by two unfriendly monsters who are called Phlogiston and Caloric, so that there are few scientific pilgrims who do not pay them toll.

The doctrine of caloric is, however, even then recognized as a chemical hypothesis rather than one acceptable to physicists, some of whom still stand out for vibratory theories even through the darkest years of the century; and, further, we may find, on strict search, that the old idea of heat as a mode of motion has not so utterly died that it does not appear here and there during the last century, not only among philosophers, but even in a popular form.

In an old English translation of Father Regnault's compilation on physics, dated about 1730, I find the most explicit statement of the doctrine of heat as a mode of motion. Here heat is defined (with the aid of a simile due, I believe, to Boyle) as "any Agitation whatever of the insensible parts. Thus a Nail which is drove into the Wood by the stroke of a Hammer does not appear to be hot, because its immediate parts have but one common Movement. But should the Nail cease to drive, it would acquire a sensible Heat, because its insensible Parts which receive the Motion of the Hammer now acquire an agitation every way rapid." We certainly must admit that the user of this illustration had just and clear ideas; and the interesting point here appears to be, that as Father Regnault's was not an original work, but a mere compendium or popular scientific treatise of the period, we see, if only from this instance, that the doctrine of heat as a mode of motion was not confined to the great men of an earlier or a later time, but formed a part of the common pabulum during the eighteenth century to an extent that has been singularly forgotten.

The last years of the eighteenth century were destined to see the most remarkable experiments in heat made in the whole of the hundred; for the memoir of Rumford appeared in the Philosophical Transactions for 1798; and in the very year 1800 appeared in the same place Sir William Herschel's paper, in which he describes how he placed a thermometer in successive colors of the solar spectrum, finding the heat increase progressively from the violet to the red, and increase yet more beyond the red where there was no color or light whatever; so that there are, he observes, invisible rays as well as visible. More than that, the first outnumber the second; and these dark rays are found in the very source and fount of light itself. These dark rays can also be obtained, he observes, from a candle or a piece of non-luminous hot iron, and, what is very significant, they are found to pass through glass, and to be refracted by it like luminous ones.

And now Herschel, searching for the final verity through a series of excellent experiments, asks a question which shows that he has truth, so to speak, in his hands, — he asks himself the great question whether heat and light be occasioned by the same or different rays.

Remember the importance of this (which the querist himself fully recognized); remember, that, after long hunting in the blindfold search, he has laid hands, as we now know, on the truth herself, and then see him — let go. He decides that heat and light are not occasioned by the same rays, and we seem to see the fugitive escape from his grasp, not to be again fairly caught till the next generation.

I hardly know more remarkable papers than these of Herschel's in the Philosophical Transactions for 1800, or any thing more instructive in little men's successes than in this great man's failure, which came in the moment of success. I would strongly recom-

mend the reading of these remarkable original memoirs to any physicist who knows them only at second-hand.

One more significant lesson remains, in the effect of this on the minds of his contemporaries. Herschel's observation is to us almost a demonstration of the identity of radiant heat and light; but now, though the nineteenth century is opening, it is with the doctrine still in the minds of most physicists, and perhaps of all chemists, that heat is occasioned by a certain material fluid. Phlogiston is by this time dead or dying, but caloric is very much alive, and never more perniciously active than now, when, for instance, years after Herschel's observation, we find this cited as "demonstrating the existence of caloric," which was, it seems, the way it looked to a contemporary.

In the year 1804 appeared what should be a very notable book in the history of our subject, written by Sir John Leslie, whose name survives perhaps in the minds of many students chiefly in connection with the 'cube,' which is still called after him.

Leslie, however, ought to be remembered as a man of original genius, worthy to be mentioned with Herschel and Melloni; and his, too, is one of the books which the student may be recommended to read, at least in part, in the original; not so much for the writer's instructive experiments (which will be found in our text-books) as for his most instructive mistakes, which the text-book will probably not mention.

He began by introducing the use of the simple instrument which bears his name, and a new and more delicate heat-measure (the differential thermometer); and with these, and concave reflectors of glass and metal, he commenced experiments in radiant heat, than which, he tells us, no part of physical science then appeared so dark, so dubious, and so neglected. It is interesting, and it marks the degree of neglect he alludes to, that his first discovery was that different substances have different radiating and absorbing powers. It gives us a vivid idea of the density of previous ignorance, that it was left to the present century to demonstrate this elementary fact, and that Leslie, in view of such discoveries, says, "I was transported at the prospect of a new world emerging to view."

Next he shows that the radiating and absorbing powers are proportional, next that cold as well as heat seems to be radiated, and next undertakes to see whether this radiant heat has any affinity to light.

He then experiments in the ability of radiant heat to pass through a transparent glass, which transmits light freely, and thinks he finds that none does pass. Radiant heat with him seems to mean heat from non-luminous sources; and the ability or non-ability of this to pass through glass is to Leslie and his successors a most crucial test, and its failure to do so a proof that this heat is not affiliated to light.

Let us pause a moment here to reflect that we are apt to unconsciously assume, while judging from our own present standpoint where past error is so plain, that the false conclusion can only be chosen by an able, earnest, conscientious seeker, after a sort of struggle. Not so. Such a man is found welcoming the false with rapture as very truth herself.

"What, then," says Leslie, "is this calorific and frigorific fluid after which we are inquiring? It is not light, it has no relation to ether, it bears no analogy to the fluids, real or imaginary, of magnetism and electricity. But why have recourse to invisible agents? *Quod petis, hic est.* It is merely the ambient AIR."

The capitals are Leslie's own, but ere we smile with superior knowledge let us put ourselves in his place, and then we may comprehend the exultation with which he announces the identity of radiant heat and common air, for he feels that he is beginning a daring revolt against the orthodox doctrine of caloric; and so he is.

The first five years of this century are notable in the history of radiant energy, not only for the work of Leslie, and for the observation by Wollaston, Ritter, and others, of the so-called 'chemical' rays beyond the violet, but for the appearance of Young's papers, re-establishing the undulatory theory, which he indeed considered in regard to light, but which was obviously destined to affect most powerfully the theory of radiant energy in general.

We are now in the year 1804, or over a century and a quarter since the corpuscular theory was emitted, and during that time it has gradually grown to be an article of faith in a sort of scientific church, where Newton has come to be looked on as an infallible head, and his views as dogmas, about which no doubt is to be tolerated; but if we could go back to Cambridge in the year 1668, when the obscure young student, in no way conscious of his future pontificate, takes his degree (standing twenty-third on the list of graduates), we should probably find that he had already elaborated certain novel ideas about the undulatory theory of light, which he at any rate promulgates a few years later, and afterward, pressed with many difficulties, altered, as we now know, to an emissive one.

Probably, if we could have heard his own statement then, he would have told how sorely tried he was between these two opinions, and, while explaining to us how the wavering balance came to lean as it did, would have admitted, with the modesty proper to such a man, that there was a great deal to be said on either side. We may, at any rate, be sure that it would not be from the lips of Newton himself that we should have had this announced as a belief which was to be part of the rule of faith to any man of science.

But observe how, if science and theology look askance at each other, it is still true that some scientific men and some theologians have, at any rate, more in common than either is ready to admit; for at the beginning of this century Newton's followers, far less tolerant than their master, have made out of this modest man a scientific pontiff, and out of his diffident opinions a positive dogma, till, as years go on, he comes to be cited as so infallible that a questioning of these opinions is an offence deserving excommunication.

This has grown to be the state of things in 1804, when Young, a man possessing something of Newton's own greatness, ventures to put forward some considerations to show that the undulatory theory may be the true one, after all. But the prevalent and orthodox scientific faith was still that of the material nature of light; the undulatory hypothesis was a heresy, and Young a heretic. If his great researches had been reviewed by a physicist or a brother worker, who had himself trodden the difficult path of discovery, he might have been treated at least intelligently; but then, as always, the camp-followers, who had never been at the front, shouted from a safe position in the rear to the man in the dust of the fight, that he was not proceeding according to the approved rules of tactics; then, as always, these men stood between the public and the investigator, and distributed praise or blame.

If you wish to hear how the scientific heretic should be rebuked for his folly, listen to one who never made an observation, but, having a smattering of every thing books could teach about every branch of knowledge, was judged by himself and by the public to be the fittest interpreter to it, of the physical science of his day. I mean Henry Brougham, the future lord-chancellor of England, the universal critic, of whom it was observed, that, "if he had but known a little law, he would have known a little of every thing." He uses the then all-powerful *Edinburgh Review* for his pulpit, and notices Young's great memoir as follows: —

"This paper contains nothing which deserves the name either of experiment or discovery; and it is, in fact, destitute of every species of merit. . . . The paper which stands first is another lecture, containing more fancies, more blunders, more unfounded hypotheses, more gratuitous fictions . . . and all from the fertile yet fruitless brain of the eternal Dr. Young. In our second number we exposed the absurdity of this writer's 'law of interference,' as it pleases him to call one of the most incomprehensible suppositions that we remember to have met with in the history of human hypotheses."

There are whole pages of it, but this is enough; and I cite this passage among many such at command, not only as an example of the way the undulatory theory was treated at the beginning of this century in the first critical journal of Europe, but as another example of the general fact that the same thing may appear intrinsically absurd, or intrinsically reasonable, according to the year of grace in which we hear of it. The great majority, even of students of science, must take their opinions ready-made as to science in general; each knowing, so far as he can be said to know any thing at first-hand, only that little corner which research has made specially his own.

The moral we can all draw, I think, for ourselves.

In spite of such criticism as this, the undulatory hypothesis of light made rapid way, and carried with it, one would now say, the necessary inference that radiant heat was due to undulations also. This was, however, no legitimate inference to those to whom radiant heat was still a fluid; and yet, in spite of all, the modern doctrine now begins to make visible progress.

A marked step is taken about 1811 by a young Frenchman, De la Roche, who deserves to be better remembered than he is, for he clearly anticipated some of Melloni's discoveries. De la Roche in particular shows that of two successive screens the second absorbs heat in a less ratio than the first; whence he, before any one else, I believe, derives the just and most important, as well as the then most novel conception, that radiant heat is of different kinds. He sees also, that, as a body is heated more and more, there is a gradual and continual advance not only in the amount of heat it sends out, but in the kind, so that, as the temperature still rises, the radiant heat becomes light by imperceptible gradations; and he concludes that heat and light are due to one simple agent, which, as the temperature rises yet more, appears more and more as light, or which, as the luminous radiation is absorbed, re-appears as heat. Very little of it, he observes, passes even transparent screens at low temperatures, but more and more does so as the temperature rises.

All this is a truism in 1888, but it is admirably new as well as true in 1811; and if De la Roche had not been removed by an early death, his would have not improbably been the greatest name of the century in the history of our subject; an honor, however, which was in fact reserved for another.

The idea of the identity of light and radiant heat had by this time made such progress that the attempt to polarize the latter was made in 1818 by Berard. We have just seen in Herschel's case how the most sound experiment may lead to a wrong conclusion, if it controverts the popular view. We now have the converse of this in the fact that the zeal of those who are really in the right way may lead to unsound and inconclusive experiment; for Berard experimentally established, as it was supposed, the fact that obscure radiant heat can be polarized. So it can, but not with such means as Berard possessed, and it was not till a dozen years more that Forbes actually proved it.

At this time, however fairly we seem embarked on the paths of study which are followed to-day, and while the movement of the main body of workers is in the right direction, it is yet instructive to observe how eminent men are still spending great and conscientious labor, their object in which is to advance the cause, while the effect of it is to undo the little which has been rightly done, and to mislead those who have begun to go right.

As an instance both of this and of the superiority of modern apparatus, we may remark, — after having noticed that the ability of obscure heat to pass through glass, if completely established, would be a strong argument in favor of its kinship to light, and that De la Roche and others had indicated that it would do so (in which we now know they were right), — that at this stage, or about 1816, Sir David Brewster, the eminent physicist, made a series of experiments which showed that it would not so pass. Ten years later, in view of the importance of the theoretical conclusion, Baden Powell repeated his observations with great care, and confirmed them, announcing that the earlier experimenters were wrong, and that Brewster was right.

Here all these years of conscientious work resulted in establishing, so far as it could be established, a wholly wrong conclusion in place of a right one already gained. It may be added, that, with our present apparatus, the passage of obscure radiant heat through glass could be made convincingly evident in an experiment which need not last a single second.

We are now arrived at a time when the modern era begins; and in looking back over one hundred and fifty years, from the point of view of the experimenter himself, with his own statement of the truth as he saw it, we find that the comparison of the progress of science to that of an army, which moves, perhaps with the loss of occasional men, but on the whole victoriously and in one direction, is singularly misleading; and I state this more confidently here, because there are many in this audience who did not get their knowledge of nature from books only, but who have searched for the truth themselves; and, speaking to them, may I not say that those who have so searched know that the most honest purpose and the most patient striving have not been guaranties against mistakes, — mistakes which were probably hailed at the time as successes? It was some one of the fraternity of seekers, I am sure, who said, "Show me the investigator who has never made a mistake, and I will show you one who has never made a discovery."

We have seen the whole scientific body, as regards this particular science of radiant energy, moving in a mass, in a wrong direction, for a century; we have seen that individuals in it go on their independent paths of error; and we can only wonder that an era should have come in which such a real advance is made as in ours.

That era has been brought in by the works of many, but more than by any other through the fact that in the year 1801 there came into the world at Parma an infant who was born a physicist, as another is born a poet; nay, more; who was born, one might say, a devotee of one department of physics, — that of radiant heat; being affected in his tenderest years with such a kind of precocious passion for the subject as the childish Mozart showed for music. He was ready to sacrifice every thing for it; he struggled through untold difficulties, not for the sake of glory or worldly profit, but for radiant heat's sake; and when fame finally came to him, and he had the right to speak of himself, he wrote a preface to his collected researches, which is as remarkable as any thing in his works. In this preface he has given us, not a summary of previous memoirs on the subject, not a table of useful factors and formulæ, not any thing at all that an English or American scientific treatise usually begins with, but the ingenuous story of his first love, of his boyish passion for this beloved mistress; and all this with a trust in us his readers which is beautiful in its childlike confidence in our sympathy.

I must abbreviate and injure in order to quote; but did ever a learned physical treatise and collection of useful tables begin like this before?

"I was born at Parma, and when I got a holiday used to go into the country the night before and go to bed early, so as to get up before the dawn. Then I used to steal silently out of the house, and run, with bounding heart, till I got to the top of a little hill, where I used to set myself so as to look toward the East." There, he tells us, he used, in the stillness of nature, to wait the rising sun, and feel his attention rapt, less with the glorious spectacle of the morning light itself than with the sense of the mysterious heat which accompanied its beams, and brought something more necessary to our life and that of all nature than the light itself.

The idea that not only mankind, but nature, would perish though the light continued, if this was divorced from heat, made a profound impression, he tells us, on his childish mind.

The statement that such an idea could enter with dominating force into the mind of a child will perhaps seem improbable to most. It will, however, be comprehensible enough to some here, I have no doubt.

Is there some ornithologist present who remembers a quite infantile attraction which birds possessed for him above all the rest of the animated creation; some chemist whose earliest recollections are of the strange and quite abnormal interest he found as a child in making experimental mixtures of every kind of accessible household fluid and solid; some astronomer who remembers when a very little creature that not only the sight of the stars, but of any work on astronomy, even if utterly beyond his childish comprehension, had an incomprehensible attraction for him?

I will not add to the list. There are, at any rate, many here who will understand and believe Melloni when he tells how this radiant heat, commonplace to others, was wonderful to his childish thought, and wrought a charm on it such that he could not see wood burn in a fireplace, or look at a hot stove, without its drawing his mind, not to the fire or iron itself, but to the mysterious effluence which it sent.

This was the youth of genius; but let not any fancy that genius in research is to be argued from such premonitions alone, unless it can add to them that other qualification of genius which has caused it to be named the faculty of taking infinite pains. Melloni's subsequent labors justified this last definition also; but I cannot speak of them here, further than to say, that after going over a large part of his work myself, with modern methods and with better apparatus,

he seems to me the man, of all great students of our subject, who, in reference to what he accomplished, made the fewest mistakes.

Melloni is very great as an experimenter, and owes much of his success to the use of the newly invented thermopile, which is partly his own. I can here, however, speak only of his results, and of but two of these, — one generally known; the other, and the more important, singularly little known, at least in connection with him.

The first is the full recognition of the fact, partly anticipated by De la Roche, that radiant heat is of different kinds, that the invisible emanations differ among themselves just as those of light do. Melloni not only established the fact, but invented a felicitous term for it, which did a great deal to stamp it on recognition, — the term 'thermochrose,' or heat-color, which helps us to remember, that, as the visible and apparently simple emanation of light is found to have its colors, so radiant heat, the invisible but apparently simple emanation, has what would be colors to an eye that could see them. This result is well known in connection with Melloni.

The other and the greater, which is not generally known as Melloni's, is the generalization that heat and light are effects of one and the same thing, and merely different manifestations of it. I translate this important statement as closely as possible from his own words. They are that

"*Light is merely a series of calorific indications sensible to the organs of sight, or Vice Versa, the radiations of obscure heat are veritable* INVISIBLE RADIATIONS *of light.*"

The Italics and the capitals are Melloni's own.

He wishes to have no ambiguity about his announcement behind which he may take shelter; and he had so firm a grasp of the great principle, that, when his first attempts to observe the heat of the moon failed, he persevered, because this principle assured him that where there was light there must be heat. This statement was made in 1843, and ought, I think, to insure to Melloni the honor of being first to distinctly announce this great principle.

The announcement passed apparently unnoticed, in spite of his acknowledged authority; and the general belief not merely in different entities in the spectrum, but in a material caloric, continued as strong as ever. If you want to see what a hold on life error has, and how hard it dies, turn to the article 'Heat,' in the eighth edition of the 'Encyclopædia Britannica,' where you will find the old doctrine of caloric still in possession of the field in 1853; and still later, in the generally excellent 'English Encyclopædia' (edition of 1867), the doctrine of caloric is, on the whole, preferred to the undulatory hypothesis. It is very probable that a searcher might find many traces of it yet lingering among us; so that Giant Caloric is not, perhaps, even yet quite dead, though certainly grown so crazy, and stiff in the joints, that he can now harm pilgrims no more.

So far as I know, no physicist of eminence re-asserted Melloni's principle till J. W. Draper, in 1872. Only sixteen years ago, or in 1872, it was almost universally believed that there were three different entities in the spectrum, represented by actinic, luminous, and thermal rays.

Draper remarks that a ray consists solely of ethereal vibrations whose lost *vis viva* may produce either heat or chemical change. He uses Descartes' analogy of the vibration of the air, and sound; but he makes no mention either of Descartes or of Melloni, and speaks of the principle as leading to a modification of views then 'universally' held. Since that time the theory has made such rapid progress, that, though some of the older men in England and on the European continent have not welcomed it, its adoption among all physicists of note may be said to be now universal, and a new era in our history begins with it. I mean by the recognition that there is one radiant energy which appears to us as 'actinic,' or 'luminous,' or 'thermal' radiation, according to the way we observe it. Heat and light, then, are not things in themselves, but whether different sensations in our own bodies, or different effects in other bodies, are merely effects of this mysterious thing we call radiant energy, without doing more in this than give a name to the ignorance which still hangs over the ultimate cause.

I am coming down dangerously near our own time, — dangerously for one who would be impartial in dealing with names of those living and with controversies still burning. In such a brief review of this century's study of radiant energy in other forms than light, it has been necessary to pass without mention the labors of such men as Pouillot and Becquerel in France, of Tyndall in England, and of Henry in America. It has been necessary to omit all mention of those who have advanced the knowledge of radiant energy as light, or I should have had to speak of labors so diverse as those of Fraunhofer, of Kirchoff, of Fresnel, of Stokes, of Lockyer, and many more. I have made no mention, in the instructive history of error, of many celebrated experimental researches; in particular of such a problem as the measurement of solar heat, great in importance, but apparently most simple in solution, yet which has now been carried on from generation to generation, each experimenter materially altering the result of his predecessor, and where our successors will probably correct our own results in time. I have not spoken of certain purely experimental investigations, like those of Dulong and Petit, which have involved immense and conscientious labor, and have apparently rightly earned the name of 'classic' from one generation, only to be recognized by the next as leading to wholly untrustworthy results, and leaving the work to be done again with new methods, guided by new principles.

In these instances, painstaking experiments have proved insufficient, less from want of skill in the investigator than from his ignorance of principles not established in time to enable him to interpret his experiments; but, if there were opportunity, it would be profitable to show how inexplicably sometimes error flourishes, grows, and maintains an apparently healthy appearance of truth, without having any root whatever. Perhaps I may cite one instance of this last from my own experience.

About fifteen years ago it was generally believed that the earth's atmosphere acted exactly the part of the glass in a hotbed, and that it kept the planet warm by exerting a specially powerful absorption on the infra-red rays.

I had been trained in the orthodox scientific church, of which I am happy to be still a member; but I had acquired perhaps an almost undue respect, not only for her dogmas, but for her least sayings. Accordingly, when my own experiments did not agree with the received statement, I concluded that my experiments must be wrong, and made them all over again, till spring, summer, autumn, and winter had passed, each season giving its own testimony; and this for successive years. The final conclusion was irresistible, that the universal statement of this alleged well-known fact (inexplicable as this might seem, in so simple a matter) was directly contradicted by experiment.

I had some natural curiosity to find how every one knew this to be a fact; but search only showed the same statement (that the earth's atmosphere absorbed dark heat like glass) repeated everywhere, with absolutely nowhere any observation or evidence whatever to prove it, but each writer quoting from an earlier one, till I was almost ready to believe it a dogma superior to reason, and resting on the well-known "*Quod semper, quod ubique, quod ab omnibus, creditum est.*"

Finally I appear to have found its source in the writings of Fourier, who, alluding to De Saussure's experiments (which showed that dark heat passed with comparative difficulty through glass), observes that if the earth's atmosphere were solid, it would act as the glass does. Fourier simply takes this (in which he is wholly wrong) for granted; but, as he is an authority on the theory of heat, his words are repeated without criticism, first by Poisson, then by others, and then in the text-books; and, the statement gaining weight by age, it comes to be believed absolutely, on no evidence whatever, for the next sixty years, that our atmosphere is a powerful absorber of precisely those rays which it most freely transmits.

The question of fact here, though important, is, I think, quite secondary to the query it raises as to the possible unsuspected influence of mere tradition in science, when we do not recognize it as such. Now, the Roman Church is doubtless quite logical in believing in tradition, if these are recommended to the faithful by an infallible guide; but are we, who have no infallible guide, quite safe in believing all we do, with our fond persuasion that in the scientific body mere tradition has no weight?

In even this brief sketch of the growth of the doctrine of radiant energy, we have perhaps seen that the history of the progress of this department of science is little else than a chapter in that larger history of human error which is still to be written and which, it is

safe to say, would include illustrations from other branches of science, as well as my own.

But — and here I ask pardon if I speak of myself — I have been led to review the labors of other searchers from this standpoint, because I had first learned, out of personal experience, that the most painstaking care was no guaranty of final accuracy; that to labor in the search for a truth with such endless pains as a man might bestow if his own salvation were in question did not necessarily bring the truth; and because, seeking to see whether this were the lot of other and greater men, I have found that it was, and that, though no one was altogether forsaken of the truth he sought (or, on the whole review of his life as a seeker, but might believe he had advanced her cause), yet there was no criterion by which it could be told at the time, whether, when after long waiting there came in view what seemed once more her beautiful face, it might not prove, after all, the mockery of error; and probably the appeal might be made to the experience of many investigators here with the question, "Is it not so?"

What then? Shall we admit that truth is only to be surely found under the guidance of an infallible church? If there be such a church, yes! Let us, however, remember that the church of science is not such a one, and be ready to face all the consequences of the knowledge that her truths are put forward by her as provisional only, and that her most faithful children are welcome to disprove them.

What then, again? Shall we say that the knowledge of truth is not advancing? It is advancing, and never so fast as to-day; but the steps of its advance are set on past errors, and the new truths become such stepping-stones in turn.

To say that what are truths to one generation are errors to the next, or that truth and error are but different aspects of the same thing to our poor human nature, may be to utter truisms; but truisms which one has verified for one's self out of a personal experience are apt to have a special value to the owner; and these lead, at any rate, to the natural question, "Where is, then, the evidence that we are advancing in reality, and not in our own imagination?"

There are many here who will no doubt heartily subscribe to the belief that there is no absolute criterion of truth for the individual, and admit that there is no positive guaranty that we, with this whole generation of scientific men, may not, like our predecessors, at times go the wrong way in a body, yet who believe as certainly that science as a whole, and this branch of it in particular, is advancing with hitherto unknown rapidity. In asking to be included in this number, let me add that to me the criterion of this advance is not in any ratiocination, not in any *a priori* truth, still less in the dictum of any authority, but in the undoubted observation that our doctrine of radiant energy is reaching out over nature in every direction, and proving itself by the fact that through its aid nature obeys us more and more; proving itself by such material evidence as is found in the practical applications of the doctrine, in the triumphs of modern photography, in the electric lights in our streets, and in a thousand ways which I will not pause to enumerate.

And here I might end, hoping that there may be some lessons for us in the history of what has been said. I will venture to ask the attention to one more, perhaps a minor one, but of a practical character. It is that in these days, when the advantage of organization is so fully recognized, when there is a well-founded hope that by co-operation among scientific men knowledge may be more rapidly increased, and when in the great scientific departments of government and elsewhere there is a tendency to the formation of the divisions of a sort of scientific army, — a tendency which may be most beneficially guided, — that at such a time we should yet remember, that, however rapidly science changes, human nature remains much the same; and (while we are uttering truisms) let us venture to say that there is a very great deal of this human nature even in the scientific man, whose best type is one nearly as unchanging as this nature itself, and one which cannot always advantageously be remodelled into a piece of even the most refined bureaucratic mechanism, but will work effectively only in certain ways, and not always at the word of command, nor always best in regiments, nor always best even under the best of discipline.

Finally, if I were asked what I thought were the next great steps to be taken in the study of radiant heat, I should feel unwilling to attempt to look more than a very little way in advance. Immediately before us, however, there is one great problem waiting solution. I mean the relation between temperature and radiation; for we know almost nothing of this, where knowledge would give new insight into almost every operation of nature, nearly every one of which is accompanied by the radiation or reception of heat, and would enable us to answer inquiries now put to physicists in vain by every department of science, from that of the naturalist as to the enigma of the brief radiation of the glow-worm, to that of the geologist who asks as to the number of million years required for the cooling of a world.

When, however, we begin to go beyond the points which seem, like this, to invite our very next steps in advance, we cannot venture to prophesy; for we can hardly discriminate among the unlimited possibilities which seem to open before a branch of knowledge which deals especially with that radiant energy which sustains, with our own being, that of all animated nature, of which humanity is but a part. If there be any students of nature here, who, feeling drawn to labor in this great field of hers, still doubt whether there is yet room, surely it may be said to them, "Yes, just as much room as ever, as much room as the whole earth offered to the first man;" for that field is simply unbounded, and every thing that has been done in the past is, I believe, as nothing to what remains before us.

The days of hardest trial and incessant bewildering error in which your elders have wrought seem over. You "in happier ages born," you of the younger and the coming race, who have a mind to enter in and possess it, may, as the last word here, be bidden to indulge in an equally unbounded hope.

A PLEA FOR LIGHT-WAVES.[1]

IT is no doubt universally conceded that no era in the world's history has ever seen such immense and rapid strides in the practical applications of science as that in which it is our good fortune to live. Especially true is this of the wonderful achievements in the employment of electricity for almost every imaginable purpose. Hardly a problem suggests itself to the fertile mind of the inventor or investigator without suggesting or demanding the application of electricity to its solution.

If we except the exquisite results obtained in the manufacture and use of diffraction gratings, and the very important work accomplished by the bolometer (a purely electrical invention, by the way), it may well be questioned whether, within the last twenty years, there has been a single epoch-making discovery or invention either in theoretical optics or in its applications.

It is mainly with a view of attempting to interest brother physicists and investigators in this to me most beautiful and fascinating of all branches of physical inquiry, that I venture to present a limited number of problems, and I think promising fields for investigation, in light, together with some crude and tentative suggestions as to their solution.

The investigations here proposed all depend upon the phenomenon of interference of light-waves. In a certain sense all light-problems may be included in this category, but those to which I wish to draw your attention are specially those in which a series of light-waves has been divided into two pencils which re-unite in such a way as to produce the well-known phenomenon of interference fringes.

The apparatus by which this is effected is known by the inconvenient and somewhat inappropriate name of 'interferential refractometer.' As the instrument which I had the honor of describing to the section at the last meeting is simple in construction, and has already proved its value in several experiments already completed and in the preliminary work of others now under way, I may be permitted to recall the chief points of its construction and theory. A beam of light falls on the front surface of a plane parallel piece of optical glass at any angle, — usually forty-five degrees, — part being reflected, and part transmitted. The reflected portion is returned by a plane mirror, normal to its path, back through the inclined plate. The second or transmitted portion is also returned by a plane mirror, and is in part reflected by the inclined plate,

[1] Abstract of an address before the Section of Physics of the American Association for the Advancement of Science, at Cleveland, O., Aug. 15-22, 1888, by Albert A. Michelson, vice-president of the section.

thus coinciding with the transmitted part of the first pencil; and the two pencils are thus brought to 'interfere.'[1] A little consideration will show that this arrangement is exactly equivalent to an air-film or plate between two plane surfaces. The interference phenomena are therefore the same as for such an air-plate.

If the virtual distance between the plane surfaces is small, white light may be employed, and we have then colored fringes like Newton's rings or the colors of a soap-film. If the distance exceeds a few wave-lengths, monochromatic light must be employed. We may confine our attention to the case of two parallel surfaces. Here it can readily be shown that the fringes are concentric circles, the common axis of the rings being the normal passing through the optical centre of the eye or telescope. Further, they are most distinct when the eye or the telescope is focused for parallel rays. In any other case we are troubled with the same perplexing changes of form and position of the fringes as already noted.

If, now, one of the mirrors have a motion normal to its surface, the interference rings expand or contract; and, by counting the fringes as they appear or disappear in the centre, we have a means of laying off any given distance in wave-lengths.

Should this work of connecting the arbitrary standard of length — the yard or the metre — with the unalterable length of a light-wave prove as feasible as it is hoped, a next step would be to furnish a standard of mass based upon the same unit.

Suppose a cube, ten centimetres on a side, with surfaces as nearly plane and parallel as possible. Next suppose a testing-instrument made of two parallel pieces of glass, whose inner surfaces are slightly farther apart than an edge of the cube. The parallelism and the distance of these surfaces can be verified to a twentieth of a wave. Now apply this testing-instrument to the three pairs of surfaces of the cube, and determine their form, parallelism, and distance to the same degree of accuracy. We have thus the means of measuring the volume of a cubic decimetre with an error less than one part in a million.

It does not seem extravagant to say that by some such plan as this we may obtain a standard kilogram which will be related to the standard of length with a degree of approximation far exceeding that of the present standard. The apparatus can also be used in the manufacture of plane surfaces, and in the measurement of co-efficients of expansion.

For all measurements of refraction and dispersion, — for solids and liquids as well as for gases, — and in the determination of the wave-length of standard lines, the accuracy of the measurement of absolute wave-lengths will depend on the accuracy with which the fixed distance can be compared with the standard metre; and this may be estimated as one part in two million.

The results of the remarkable work of Rowland do not claim a much greater degree of accuracy than one part in half a million for relative determinations; while the elaborate research of Bell on absolute wave-lengths claims but one in two hundred thousand.

It may possibly help to realize the very considerable superiority of this instrument over the grating — at any rate, for the class of work in question — if I recall to your attention the fact that by its means it has been possible to show that the red line of hydrogen is a very close double.

Closely connected with the preceding investigations is the study of the effect of the temperature, thickness, and density of the source on the composition of the radiations, as shown by the symmetrical or unsymmetrical broadening of the spectral lines, and the consequent shifting of their mean position. This question has quite recently been taken up by H. Ebert, and the results he has already obtained are very promising. Ebert has established two conclusions, which, if verified, are of the greatest importance: namely, first, that the chief factor in the broadening of the spectral lines is the increase in density of the radiating body; second, that the broadening, in all the cases examined, is unsymmetrical, causing a displacement of the line toward the red end of the spectrum. The importance of these conclusions, in their relation to the proper motions of the heavenly bodies and their physical condition, can hardly be overestimated. The value of results of this kind would, however, be much enhanced if it were possible to find a quantitative relation between the density of the radiating substance and the nature of its radiations. In the case of hydrogen enclosed in a vacuum tube this could readily be accomplished. It may, however, be objected that it would be difficult in this case to separate the effects of increased density from those due to the consequent increase in the temperature of the spark. The problem of the temperature of the electric discharge in rarefied gases is one which has not yet been solved. In fact, it may seriously be questioned whether in this case temperature has any thing to do with the accompanying phenomena of light; and it appears to me much more reasonable to suppose that the vibratory motion of the molecules is not produced by collisions at all, but rather by the sudden release of tension in the surrounding ether.

[1] A second plane parallel plate of the same thickness and inclination is placed (for compensation) in the path of the first pencil.

BOOK-REVIEWS.

The Philosophy of Kant. By JOHN WATSON. New York, Macmillan. 8°. $1.75.

THE present volume consists of a number of extracts from Kant's principal works, — 'The Critique of Pure Reason,' 'The Metaphysic of Morality,' 'The Critique of Practical Reason,' and 'The Critique of Judgment,' — and is intended for the use of teachers of philosophy. Undoubtedly the study of Kant is the best introduction into modern philosophy, and a powerful means of guarding students from falling into a shallow materialism or positivism. The extracts are well selected, and the difficult task of rendering Kant into intelligible English without altering the character of his style too much has been skilfully solved. The book is an enlarged edition of the author's 'Extracts from Kant's Writings,' which was originally printed for the use of his own students. Professor Watson says that he found by experience the results obtained by means of lectures on philosophy very unsatisfactory, as the students did not learn to think for themselves: therefore he adopted the plan of supplementing his lectures by the study of the writings of various philosophers. This is the same method which is so successfully followed at German universities in what are called 'seminaries.' The teacher who will take this course will find Watson's book very useful and convenient, as it contains the salient points of Kant's philosophy.

Latin Accidence and Exercises. By W. WELCH and C. G. DUFFIELD. London and New York, Macmillan. 24°. 40 cents.

THIS book is intended as an introduction to Macmillan's 'Elementary Classics.' The principles on which the authors' plan is based are a thorough and accurate mastery of the elements of the Latin language, and the putting into intelligent practice at once what has been learned, thus avoiding as much rote-work as possible. The examples have been taken largely from the 'Public Schools Latin Primer,' as the latter is most widely used in the higher forms. The authors do not deem it desirable that beginners should learn the conjunctive mood, which, for this reason, has been added in small type at the end of the 'Accidence.' The book is intended to be mastered in two terms.

Elementary School Atlas. By J. BARTHOLOMEW. London, Macmillan. 8°. 30 cents.

THE publication under review belongs to Macmillan's Geographical Series, edited by A. Geikie, who promoted the interests of teaching geography so well by his well-known essay on this subject. As might be expected, the atlas represents a great improvement upon the ordinary English elementary school-maps, the material which is embodied in the maps being carefully selected, and the abominable relief-plate printing being at last discarded, a clear lithograph taking its place. The atlas contains twenty-four maps or plates. The first shows a number of hemispheres: the northern and southern (land and water) and the European and South American. We would gladly miss the last, as it is intended only to show the central position of Europe. The second map is named 'Europe, illustrating Geographic Terms.' This map must be considered a failure, as it attempts the explanation of geographic terms, instead of by means of objects, by that of a highly and wrongly generalized map. The following plate, which illustrates the mapping of a landscape and the influence of reduction, ought

to precede the former, and we believe greater care in its technical execution would have been desirable. As the map is intended to explain the meaning of hill-shading, the view of the hills and the map ought to be clear, and it ought to be possible to compare them down to minute details. The fourth plate explains the system of meridians and parallels and the curvature of the earth's surface. The rest of the maps are well selected, and do not call for any special comment. The maps of the British Isles are very good. We think, however, that a hypsometric map like No. 11 is of no great value for educational purposes, as contour-lines, unaided by hill-shading, do not convey to the child a good idea of the physical features of a country. Considered as a whole, the atlas must be commended as a great improvement upon the ordinary school atlas.

NOTES AND NEWS.

THE United States Fish Commission is undertaking an extensive series of explorations of the fish fauna of the rivers of the Alleghany region. The work is in charge of Prof. D. S. Jordan, assisted by Prof. P. P. Jenkins, Prof. B. W. Evermann, and Mr. Barton A. Bean. The basins of the James, Kanawha, Roanoke, Holston, French Broad, Yadkin, and Catawba will be included in the work of the present summer. Similar explorations of the smaller lakes of Michigan are under direction of Mr. Charles H. Bollman.

— The fourth article in the Railway Series now appearing in *Scribner's Magazine* will be contributed to the September number by Gen. Horace Porter, who writes of 'Railway Passenger Travel.' —— 'The Record of a Human Soul' is the title of an anonymous little book to be published shortly by Longmans, Green, & Co. It is the honest account of the struggle of a sceptic, who ardently but unavailingly desired to believe, from the coming of the doubt until the hour when the doubter at last sees a light in heaven. It is introspective and subtle, but not morbid; its language is simple and direct; and the record is likely to be useful to not a few who have only the honest doubt in which there may be more faith than in half the creeds.

— The Canadian Institute, Toronto, Ont., is desirous of collecting, and incorporating in its Proceedings, reliable data respecting the political and social institutions, the customs, ceremonies, beliefs, pursuits, modes of living, habit, exchange, and the devolution of property and office, which obtain among the Indian peoples of the Dominion. It feels that this department of research has not been so fully cultivated in Canada as its importance demands, fears that the opportunity of gathering and carefully testing the necessary facts may with the advancing tide of European civilization soon pass away, and is of opinion that much light may be cast upon the genesis and growth of government as well as upon legal, sociological, and economic thought by an accurate study of the Indian tribes in their existing conditions and organizations. Contributions to the philology of the Indian tongues, and additions to their folk or myth lore, will be welcomed as heretofore. At the same time the institute begs leave, without desiring to contract the field of observation, to direct attention to the sociological matters.

— A new process for protecting iron against corrosion, now employed by a company at Port Chester, N.Y., is said to give satsfactory results. The company is now manufacturing sanitary soil-pipes treated by this method, which is described by Mr. H. Haupt as follows: "After the pipes have been lowered into the retorts by means of a traveller, the retorts are closed for about fifteen minutes until the contents are heated to the proper temperature. Steam from a boiler at sixty pounds pressure is then introduced into the superheater, which it traverses, and from which it escapes at the temperature of the iron, upon which it acts for about one hour. A measured quantity of some hydrocarbon is then admitted with a jet of steam, followed again by a fixing bath of superheated steam, which completes the process." Professor Gesner, the director of the works, says there is no pressure in the retort, and that there are no free explosive gases. The water-seals attached to the retorts show only slight oscillations, but not an inch of pressure; and when the covers are removed and air admitted there is no explosion, as there always is when free hydrogen or carbonic oxide is present. The absence of pressure and of explosive gases is a proof that all the operations have been so nicely regulated as regards material used, quantity, and time of application, that a perfect absorption and union of the carbon, oxygen, and hydrogen with the iron has been effected. The protection thus afforded to the iron is not a mere coating, like paint, but is said to be an actual conversion, to a greater or less depth, into a new material. When properly treated, this material does not seem to be detachable by pounding, bending, hammering, rolling, or heating. The pipes treated at Port Chester have been immersed in baths of dilute sulphuric acid and exposed to the salt air for weeks without change, while untreated pipes were quickly covered with red oxide, or with sulphate of iron.

LETTERS TO THE EDITOR.

Re-appearance of Song-Birds.

THE appearance of birds is always quite irregular, so far as numbers are concerned, with the possible exception of one or two varieties like the migratory thrush. We will find in any locality that the oriole is very plentiful for a few years, and then comparatively scarce for a few years. This cannot be mistaken by those on whose gardens he makes his inroads. The absence of grossbeaks and then their great abundance is equally marked. So of nearly all familiar birds. The cause is probably that they range over a large territory, and select different nesting-centres. It is well known that pigeons will cover the sky for two or three springs, moving to a camp in the farther north, and then for years not a pigeon be seen. I believe my catbirds alone have so taken to me that I can always count on their familiar forms and delicious notes.

The extraordinary abundance of song-birds is no doubt a simple coincidence or accidental agreement of action on the part of several species. In my own grounds I do not see any such unusual migration; for the reason, probably, that I have for many years so protected and fed them, that it is a paradise for birds. Yet it is true that several sorts of birds are on the increase here; owing, possibly, to finding their quarters disturbed elsewhere. The line of migration can be much more easily swerved than the ponderous and slow movements of animals. I think you may be sure that the abundance in some quarters is balanced by the deficit in other quarters. New influences constantly arise, affecting the peace and content of birds. I have all summer been fighting a band of pseudo-scientists; that is, boys who carry papers permitting them to shoot our birds to make collections for so-called scientific purposes. Before the law to protect our song-birds, no decent young man would prowl about near our residences to shoot the pets. But now they are 'scientists;' and we have no rights to be considered. They crack their guns under our very noses. But I have vowed to have a lawsuit with every budding Audubon that comes this way, and am at present ahead.

Now, here is a law that works not at the muzzle, but the but. Its effect is to scatter our birds in their favorite haunts. My grounds cover nine acres only, but several neighbors are in full accord; and there are full fifty acres of flowers, hedges, and fruit where the song-birds are wonderfully abundant. But how long would they remain with us if one after another fell victims the moment they flew outside our lines? Another year we should lament the absence of our birds, and somewhere else people would rejoice in their superabundance.

E. P. POWELL.

Clinton, N.Y., Aug. 7.

The Physical Aspect of the Planet Mars.

THERE has been so much said of late, in the newspapers and elsewhere, in regard to the parallel canals of Mars, that perhaps a brief discussion of the facts observed in regard to them may be of interest. And first of all it may be remarked, that, of all the different methods of accounting for the appearances observed, perhaps the least probable is that they are water-canals.

Let us see what are the facts in the case. According to the observations of Schiaparelli (*Reale Accademia dei Lincei* 1881 and 1886) they lie almost entirely between 50° north and south latitude (that is, in the torrid and warmer portions of the temperate zones), and extend across the continent from the northern to the southern

ocean. They are in general two or three thousand miles in length, though sometimes much longer, by from perhaps thirty to one hundred and fifty miles in breadth. They are generally arranged in pairs two or three hundred miles apart, drawn on the arcs of great circles, and so exactly parallel that usually no deviation can be detected. They run in all directions, but there are about a dozen points which seem marked as special centres from which they radiate. Thus ten start from the Trivium Charontis as a centre, and eight from the Lacus Phœnicis. They cut up the continental surface of the planet so that there is no spot more than four hundred miles distant from one of these markings. They are usually so fine that no color can be assigned to them, and they can be merely spoken of as dark lines; but in a few instances where they broaden out, as in the Lyrtis Major (if this conspicuous marking can be considered one of them), they are decidedly darker than the oceans, and of a grayish or perhaps greenish tint.

Of a well-defined canal called by Schiaparelli, Hades, M. Perrotin (*Annales de l'Observ. de Nice*, c. 58) remarks, "Since our first observations, the canal *LN* has suffered a considerable change: we can distinguish it no longer save to a feeble extent on the side marked *N*. Though drawn on the map of M. Schiaparelli of 1882, this canal does not exist on that of 1879. Our observations, then, not only confirm the changes already stated, but they show further that these changes may be produced in a short period of time." Other evidences of change have since been observed. It is thought that a large portion of the red region known as Libya had changed to green, and afterwards in part back to red. But the latest evidence of change, according to M. Perrotin (*Comptes Rendus*, cvi. 1718, and cvii. 161), is the carrying of several of the so-called 'canals' across the northern ocean up to the polar ice-cap. If the observation is correct, it is clear that either the ocean is not an ocean, or the canals are not canals. If the observation were confirmed, I should be inclined to deny both propositions. Indeed, the northern ocean as represented by M. Perrotin at this point is but little more than an enlarged canal, while M. Schiaparelli does not indicate it at all upon his maps.

The latter has thought that many of these canals appear only for a short time, and then disappear again; and some of them he has only seen shortly after the passage of the vernal equinox on Mars, and thinks that there may be some relation between the two.

To every argument as to the inherent improbability of an hypothesis made with regard to a remote planet, we may be met by the statement that under different conditions these very things may happen, — a statement easily made, and hard to refute. The best we can do, however, is to reason by means of the laws which we have found to apply in the case of the earth. Certainly no such straight canals could be made here naturally, and, if they were made, they would soon be filled up again. If, on the other hand, the canals were artificial, what could be the use of making them so wide, why arrange them always in duplicate, and why fill certain of them up every year, later to be re-opened? Think of the labor involved in covering over, and then re-opening, a canal, say, sixty miles wide by three thousand miles long, and all in the space of a few weeks. Moreover, in the case of those which are sufficiently wide for us to see distinctly, why should the color be so much darker than that of the neighboring oceans?

Mr. R. A. Proctor has suggested (*Monthly Notices Roy. Ast. Soc.*, xlviii. 307) that the canals are the diffraction-images of rivers produced by mist which hangs over the river-beds. To this suggestion, however, some of the same objections apply as to the other.

M. Fizeau's suggestion (*Comptes Rendus*, cvi. 1759), that the stripes are cracks between huge masses of ice, presents some difficulty in accounting for the red color of the ice; and also, as was pointed out by M. Flammarion (*Comptes Rendus*, cvii. 19), since the temperature of Mars as indicated by the size of the polar spots, is, if any thing, higher than that of the earth, it is surprising that the ice does not melt.

Before going further let us see what is the probability that these supposed markings are really genuine. Several astronomers, — Dawes, Perly, Burton, and others — have independently constructed maps of Mars, or made observations from which such maps could be constructed; and it is found, on comparison, that a number of these stripes are common to several of the maps. It is therefore probable that these particular stripes are really there. M. Perrotin has confirmed the doubling of the stripes previously mapped by M. Schiaparelli: it is therefore quite possible that these are genuine also, although the observation is one of extreme difficulty, requiring the steadiest possible atmospheric conditions. But the statement that a change in the markings has been observed is one that must be received with extreme caution, and, although a most interesting one, must for this very reason be only accepted as proved, when confirmed by observations made with the most powerful telescopes at our disposal, and under the most favorable circumstances.

Starting out from the generally accepted fact that there are stripes upon the planet, we find there are five possible hypotheses to explain their existence. Three, that they are due to water in the vaporous, liquid, or solid condition, we have already noticed. Fourth, we may explain them by supposing them to depend on the color of the rock or soil, and that their shape depends on some peculiar geological formation. We have, to be sure, no such formations upon our globe; but we have something analogous, though on a somewhat smaller scale, upon the moon. There we find numerous long narrow streaks radiating from the crater Tycho, as also in a lesser degree from some of the other craters. The streaks are perfectly straight, of very light color, and in a few cases we find them arranged parallel to one another.

As to the color of Mars, it is probable that the earth would appear of the same color as seen from a distance, if deprived of its vegetation, owing to the red color of its soil in most parts of the world, particularly in the warmer regions.

If it can be shown, however, that at certain seasons the stripes on Mars really disappear, through some other cause than that of passing clouds or haze in its own atmosphere, then this hypothesis, like that of the water-canals, must fail.

The fifth and last of the possible explanations is that the stripes are due to differences in vegetation. Whether the stripes indicate vegetation, and the rest is a barren waste, or whether a large proportion of the vegetation of Mars is of a reddish color, as suggested by Lambert among others, and approaches in tint to our coleus and autumn leaves, is a matter of no consequence at present. If it can be shown that the stripes on Mars really change, this will be the hypothesis that we shall be forced to adopt, or, rather, we should say it is the only one left presenting no serious improbabilities.

Let us now review the already ascertained facts with regard to the planet. We are reasonably certain that the surface of Mars is composed of land and water; that it has snow at its poles, and therefore an atmosphere containing clouds. As the snow does not extend over the whole planet, but varies in extent at different times, and what are apparently clouds have been observed in other regions of its surface, it is probable that they likewise have rain. Their temperature cannot be very different from ours, judging by the extent of the snow at the poles, which is rather less in proportion than with us, and has in some instances been known to entirely disappear. Their days are but forty minutes longer than ours; and their seasons, owing to the inclination of the axis of Mars, are practically the same. The most marked difference between the two planets, of which we are certain, is, that, owing to the lesser attraction of Mars, bodies there would weigh but two-fifths as much as with us: a man, for instance, weighing one hundred and sixty pounds here, would weigh but sixty-four pounds upon Mars. All the conditions as far as we can determine, save that their sunlight is somewhat weaker, are as favorable to the growth of organic life there as here.

The spectroscope teaches us that the same elements are found throughout the universe: therefore, if we define vegetable life as consisting of organized structures absorbing carbonic acid and giving out oxygen, it will be seen that the admission that vegetable life exists upon Mars carries with it animal life also as a corollary, or vegetation would soon cease for lack of fresh air.

As Mars is a smaller planet than the earth, and more remote from the sun, it probably reached a suitable temperature to support organic life at an earlier date. The laws of evolution have therefore had sufficient time to develop reasonably highly organized animal as well as vegetable life.

This is as far as we are justified in carrying our hypothesis, unsupported by other facts; but now let us give rein to our fancy for

a moment, and suppose an observer on Mars were to examine the earth with successive increasingly powerful telescopes. The first artificial production that he would probably be able to see would be some of the great grain-fields of our Western States. These he would find of irregular shape, but bounded more or less by straight lines. They would appear of a greenish color, not very different from that of our oceans; and he would find them subject to great changes at certain seasons, sometimes perhaps entirely disappearing from sight, when of the same tint as the surrounding country. In fact, if an observer were placed on Mars, and furnished with one of our more powerful telescopes, he would see just about as much of our grain-fields as we do of their stripes, and the only noticeable difference between the two would lie in their shape. Indeed, assuming an artificial origin, it would be easy to frame hypotheses accounting for their form, dependent upon the peculiar conformation of the land surface of Mars, or for their radiating in several instances from particular points as centres.

But to return to our hypothesis, that the stripes are of vegetable origin. If it is correct, there is one test to which it must submit. If a change is noted in a given stripe, this change should be in general more or less progressive from the equator towards the poles, or *vice versa*. I say in general, because it is not probable that the same kind of vegetation would exist all the way from the equator to 50° north or south latitude, nor would it be the same in all stripes having the same latitude. Moreover, in the stripes running east and west, or in those situated near the equator, successive changes would not usually be noticeable. Stripes containing the same kind of vegetation should be similarly affected. Now, in the stripe known as Hades, previously referred to, this very phenomenon was observed. Hades runs in a direction nearly north and south, and extends from latitude 20° to 45° north. The observation in question was made about two and a half of our months after the passage of the northern solstice on Mars. It was therefore in the latter part of their summer when it was found that the southern portion of what had but a few weeks before been a well-defined stripe had completely disappeared.

As an illustration of the formation of a stripe running from the equator towards the pole, let us take the latest observations of M. Perrotin (*Comptes Rendus*, cvii. 161). According to these observations in the regions as far north as between latitudes 50° and 60°, the stripes did not appear this year until June 4, or four months after the summer solstice. Unfortunately, Mars is now getting so near the sun that it will be probably impracticable to determine the date of their disappearance, should they be found later to have vanished. WM. H. PICKERING.

Observatory, Cambridge, Mass., Aug. 9.

The Philippine Islands.

MR. WALLACE, in his great work, 'The Geographical Distribution of Animals,' divides the Oriental or Indian region of Mr. Sclater into four sub-regions, of which Java, Sumatra, Malacca, Borneo, and the Philippine Islands form one, which he calls the Indo-Malayan. In his discussion of the Indo-Malayan sub-region, Mr. Wallace recognizes several subdivisions of it, and treats of the Philippine group as one of the most important of these. Though acknowledging the existence of divisions of his sub-regions, he failed to give them technical names, as at that time uncalled for. The purpose of this paper is to show that the Philippines themselves are separated into several distinct zoölogical divisions; and it therefore seems necessary, for their study, to give technical names to the primary and secondary divisions of the already recognized sub-regions. The terms 'province' and 'sub-province' seem least objectionable, and will be used here; the Philippine Islands thus forming one of the provinces of the Indo-Malayan sub-region, and the divisions of the group itself sub-provinces.

The zoölogical province of the Philippines is co-extensive with the political division of the same name, with the exception, perhaps, of the islands of Sulu and Tawi Tawi, which lie between the Philippines and Borneo, but are claimed by the Spanish.

The sub-provinces proposed are, first, the northern Philippines, consisting of Luzon, Marinduque, and a number of other small islands about Luzon; second, Mindoro; third, the central Philippines, made up of the islands of Panay, Guimaras, Negros, Cebu, Bojol, and Masbate; fourth, the eastern Philippines, comprising the islands of Samar and Leite; fifth, the southern Philippines, embracing the great island of Mindanao, with Basilan; and, sixth, the western Philippines, consisting of the islands of Paragua or Palawan, and Balabac.

The geographical positions of these sub-provinces are fortunately such that these simple names show their relation to each other very closely, as may be seen by consulting a map of the archipelago.

Of these sub-provinces, the western Philippines, made up of Paragua and Balabac, and perhaps the Calamianes, is of most importance, its animal life being much more closely allied to that of Borneo than that of any other sub-province of the group. This is especially noticeable in its mammals, of which it possesses, in common with Borneo, the genera *Tragulus*, *Tupaia*, and *Manis*, which are apparently absent from the rest of the archipelago. Of Bornean genera of birds not found elsewhere in the group, *Jora*, *Criniger*, *Polyphetron*, *Tiga*, and *Batrachostomus* are examples The sub-province has evidently received a large part of its fauna from North Borneo, through Balabac, and at a comparatively recent date, and since its separation on the north from the rest of the Philippines, so that these genera have not flown over into Mindoro and Luzon. In addition to these apparently late arrivals from Borneo, the sub-province possesses a large number of peculiarly Philippine birds and mammals, which show that it is an integral part of the province.

The rest of the Philippines would seem to have received its Malayan fauna at another time and by the other way of Sulu and Mindanao. They possess the mammalian genera *Galeopithecus*, *Tarsius*, and *Cervus*, which are apparently wanting to the western sub-province, and the genera *Macacus*, *Sus*, *Viverra*, *Paradoxurus*, and *Sciurus* in common with it. Of birds, the genera *Loriculus*, *Cyclopsitta*, *Buceros*, and *Penelopides* are examples which are more or less generally distributed over the archipelago outside of the western sub-province.

The grounds for dividing the Philippines east of Paragua into sub-provinces are to quite an extent based upon species, and especially upon the existence in each of representative forms of the genera *Loriculus*, *Buceros*, *Penelopides*, *Brachiurus*, *Chrysocolaptes*, *Dicæum*, *Cinnyris*, etc. The hornbills form, perhaps, the most striking example of this distribution of representative species. Of the eleven species of hornbills collected in the islands, the western sub-province has one, the southern three, the central two, the eastern two, Mindoro one, and the northern two; and we have found no case of a single species occupying more than one sub-province, or of more than one species of a genus in a single sub-province. The genera *Chrysocolaptes* of woodpeckers is also noticeable, each sub-province possessing its own species, with the exception of Mindoro, which apparently lacks the genus altogether. The genus *Loriculus* of the parrots is of the same character.

Of other animals than birds, the genus *Sciurus* of mammals, and *Draco*, the flying lizards, seem to have representative species in each sub-province, and the land mollusca are probably distributed according to the same plan.

The above examples are a few that come to mind before a careful study of our collections has been made, and they do not by any means represent all the reasons for the conclusions arrived at. These are the result, rather, of the observation of five careful men who have been collecting and studying in the Philippines during the last year. During this time we have visited and collected in fifteen of the islands of the group, and these the largest and most important. I am satisfied that the study of our collections, with the aid of the libraries and collections at home, will only strengthen the conclusions of this paper. It may prove necessary to make the so-called western sub-province of more importance in the arrangement, but the non-existence in nature of exactly equivalent divisions of any kind is well recognized. It is hoped that our work may aid in untangling some of those puzzles in which students of Philippine zoölogy have found themselves involved, and that it will also add considerably to the sum of knowledge concerning this as yet imperfectly known corner of the earth. J. B. STEERE.

Manila, July 2.